

XXI.—*On the Path of a Rotating Spherical Projectile.* By Prof. TAIT.
(With a Plate.)

(Read 5th June and 3rd July 1893.)

The curious effects of rotation upon the path of a spherical projectile have been investigated experimentally by ROBINS and many others, of whom MAGNUS is one of the more recent. They have also been the subject of elaborate mathematical investigation, especially by POISSON, who has published a large treatise on the question.* For all that, we know as yet very little more about them than NEWTON did in 1666, when he made his famous experiments on what we now call dispersion. Writing to OLDENBURG an account of these experiments in 1671–2,† he says :—

“Then I began to suspect whether the rays, in their trajection through the prism, did not move in curve lines, and according to their more or less curvity, tend to divers parts of the wall. And it increased my suspicion, when I remembered that I had often seen a tennis-ball, struck with an oblique racket, describe such a curve line. For, a circular as well as a progressive motion being communicated to it by that stroke, its parts, on that side where the motions conspire, must press and beat the contiguous air more violently than on the other ; and there excite a reluctancy and re-action of the air proportionably greater. And for the same reason, if the rays of light should possibly be globular bodies, and by their oblique passage out of one medium into another acquire a circulating motion, they ought to feel the greater resistance from the ambient æther, on that side where the motions conspire, and thence be continually bowed to the other.”

From this remarkable passage it is clear that NEWTON was fully aware of the effect of rotation in producing curvature of the path of a ball, also that it could be of sufficient amount to be easily noticed in the short flight of a tennis-ball ; that he correctly described the direction of the deviation, and that he ascribed the effect to difference of air-pressure for which he assigned a cause. All that has since been done experimentally seems merely to have given various more or less striking illustrations of these facts, without any attempt to find how the deflecting force depends upon the velocities of translation and rotation : and I am not aware of any successful attempt to extend or improve NEWTON's suggestion of a theoretical explanation. It seems in fact to have been altogether unnoticed, perhaps even ignored.

Thus ROBINS,‡ writing some seventy years later than the date of NEWTON's letter, speaks of

“the hitherto unheeded effects produced by this resistance ; for its action is not

* *Recherches sur le Mouvement des Projectiles dans l'Air.* Paris, 1839.

† *Isaac Newtoni Opera quæ exstant Omnia* (Horsley), vol. iv. p. 297.

‡ *New Principles of Gunnery* (new edit.), 1805, p. 206. The paper referred to is stated to have been read to the Royal Society in 1747.

solely employed in retarding the motions of projectiles, but some part of it exerted in deflecting them from their course, and in twisting them in all kinds of directions from their regular track ; this is a doctrine, which, notwithstanding its prodigious import to the present subject, hath been hitherto entirely unknown, or unattended to ; and therefore the experiments, by which I have confirmed it, merit, I conceive, a particular description ; as they are themselves too of a very singular kind."

ROBINS measured accurately, by means of thin screens placed across his range, the deviation (to right or left) of successive shots fired from a gun which could be exactly replaced in its normal position, after each discharge ; and found that it increased much more rapidly than in simple proportion to the distance. Then he experimented successfully with a gun whose barrel was bent a little to the left near the muzzle, with the view of forcing a loose-fitting bullet to rotate by making it roll on one side of the bore. The bullet, of course, at first deviated a little to the left ; but this was soon got over, and it then persistently curved away to the right. And he showed the effect of rotation very excellently by suspending a ball by two strings twisted together, so as to give rotation to it when it was made to vibrate as a pendulum. The plane of vibration rotated in the same sense as did the ball.

I have not had an opportunity of consulting, in the original, EULER's remarks on this question. The following quotations are taken from a retranslation* of his German version of ROBINS' work, but the statements they contain are so definite that the translator cannot be supposed to have misrepresented their meaning :—

"The cause which Mr ROBINS assigns for the uncertainty of the shot cannot be the true one, since we have indisputably proved, that it arises from the figure of the ball only." p. 313.

"if the ball has a progressive motion, we may, as has been already shewn, consider it at rest, and the air flowing against it with the velocity of the ball's motion ; for the force with which the particles of air act on the body will be the same in both cases." [Then follows an investigation.] "hence this proposition appears indisputably true ; that a perfectly spherical body which, besides its progressive motion, revolves round its centre, will suffer the same resistance as if it had no such rotation. If, therefore, such a ball should receive two such motions in the cannon, yet its progressive motion in the air would be the very same as if it had no rotation." pp. 315-7.

POISSON's treatment of the subject is altogether unnecessarily prolix, and in consequence not very easily understood. It is sufficient to say that, like EULER, he rejects† ROBINS' explanation ; and that his basis of investigation of the effects of rotation on the path of

* "The true Principles of Gunnery investigated and explained, comprehending translations of Professor EULER's Observations, &c. &c." By Hugh Brown. London, 1277 (*sic*).

† POISSON, in fact, says of his own results :—"Néanmoins, d'après la composition de la formule qui exprime la déviation horizontale à la distance du canon où le boulet retombe sur le terrain, on reconnaît facilement que cette déviation ne peut jamais être qu'une très petite fraction de la longueur de la portée ; en sorte que ce n'est pas au frottement de la surface du boulet contre la couche d'air adjacente et d'inégale densité, que sont dues principalement les déviations observées, ainsi que Robins et Lombard l'avait pensé." *Mémoire sur le Mouvement des Projectiles, &c. Comptes Rendus*, 5 Mars, 1838, p. 288.

a homogeneous sphere really amounts to no more than this:—that, since friction is greater where the density of the air is greater, the front of the ball suffers greater friction than does the back. Thus there is a lateral force, which he shows to be very small, tending to deflect the ball as if it were rolling upon the air in front of it. As this is exactly the opposite of the effect described by ROBINS, I feared at first that I must have misunderstood POISSON'S mathematics. But this feeling gave way to one of astonishment when I read further; for there can be no doubt of the meaning of the following passage which occurs in his comments on the investigation:—

“C'est ce que l'on peut aussi regarder comme évident *à priori*, si l'on considère que cette déviation est due à l'excès de la densité de l'air en avant du projectile, sur sa densité en arrière; excès qui donne lieu à un plus grand frottement du fluide, contre l'hémisphère antérieur, et à un moindre contre l'hémisphère postérieur . . . il en résultera une force horizontale qui poussera ce point [the centre of inertia] dans le sens du plus grand frottement ou en sens contraire de la rotation à laquelle il répond, c'est-à-dire vers la gauche, quand les points de la partie antérieure du projectile tourneront de gauche à droite, et vers la droite, lorsqu'ils tourneront de droite à gauche.” *Recherches, &c.*, p. 119.

In fact, POISSON'S elaborate investigation leads to no term, in the expression for the normal component of the force, which can have different values at corresponding points of the two front semihemispheres of the projectile:—and it is to a force of *this* nature that NEWTON'S remarks and ROBINS' experiments alike point.

The paper of MAGNUS* commences with a historical sketch of the question, but it contains no reference to NEWTON. The author obviously cannot have read ROBINS' papers, for he mentions his work only once, and in the following altogether inadequate and unappreciative fashion:—

“Robins, der zuerst eine Erklärung dieser Abweichung in seinen *Principles of Gunnery* versucht hat, glaubte, dass die ablenkende Kraft durch die Umdrehung des Geschosses erzeugt werde, und gegenwärtig nimmt man dies allgemein an.”

Had MAGNUS known of the experiments with the crooked gun-barrel and the rotating pendulum, he would surely have employed a stronger expression than “glaubte”! For ROBINS says (p. 208) of his own pendulum experiment:—

“it was always easy to predict, before the ball was let go, which way it would deflect, only by considering on which side the whirl would be combined with the progressive motion; for on that side always the deflecting power acted; as the resistance was greater here, than on the side where the whirl and progressive motion were opposed to each other.”

This passage strongly resembles part of the extract already made from NEWTON'S letter. But ROBINS justly adds (two words have been italicized)—

“This experiment is an *incontestible proof*, that, if any bullet, besides its progressive motion, hath a whirl round its axis, it will be deflected in the manner here described.”

* Über die Abweichung der Geschosse, *Berlin Trans.*, 1852.

The one novelty in the experiments of MAGNUS (so far as *spherical* projectiles are concerned) consisted in blowing a stream of air against the rotating body, instead of giving it a progressive as well as a rotatory motion; thus, in fact, realizing the idea suggested by EULER in one of the quotations made above. He was thus enabled, by means of little vanes, to trace out in a very interesting and instructive manner the character of the relative motion of the air and the rotating body. This was a cylinder instead of a sphere, so the effects were greater and of a simpler character, but not so directly applicable to bullets. Otherwise, his experiments are merely corroborative of those of ROBINS.

But neither ROBINS nor MAGNUS gives any hint as to the form of the expression for the deflecting force, in terms of the magnitudes of the translatory and the rotatory speed. That it depends upon *both* is obvious from the fact that it does not exist when either of them is absent, however great the other may be.

1. For some time my attention has been directed to this subject by the singularly inconsistent results which I obtained when endeavouring to determine the resistance which the air offers to a golf-ball.* The coefficient of resistance which I calculated from ROBINS' data for iron balls, by introducing the mass and diameter of a golf-ball, was very soon found to be too small:—and I had grounds for belief that even the considerably greater value, calculated in a similar way from BASHFORTH's data, was also too small. Hence the reason for my attempts to determine its value, however indirectly. The roughness of the ball has probably considerable influence; and, as will be seen later, so possibly has its rotation. I collected, with the efficient assistance of Mr T. HODGE (whose authority on such matters, alike from the practical and the observational point of view, no one in St Andrews will question) a fairly complete set of data for the average characteristics of a really fine drive:—elevation at starting, range, time of flight, position of vertex, &c. Assuming, as the definite result of all sound experiment from ROBINS to BASHFORTH,† that the resistance to a *spherical* projectile (whose speed is less than that of sound) varies nearly as the square of the speed, I tried to determine from my data the initial speed and the coefficient of resistance, treating the question as one of ordinary *Kinetics of a Particle*. We easily obtain, for a low trajectory, simple but sufficiently approximate expressions for the range, the time of flight, and the position of the vertex, in terms of the data of projection and the coefficient of resistance. If, then, we assume once for all an initial elevation of 1 in 4, the only disposable initial element is the speed of projection. Making various more or less probable assumptions as to its value, I found for each the corresponding coefficient of resistance which would give the datum range. Thus I obtained the means of calculating the time of flight and the position of the vertex of the path. The greater the assumed initial speed (short, of course, of that of sound) the larger is the coefficient of resistance required to give the datum range, and the more

* "The Unwritten Chapter on Golf," NATURE, 22/9/87; and "Some Points in the Physics of Golf," IBID., 28/8/90, 24/9/91, 29/6/93. Also a popular article "Hammering and Driving," GOLF, 19/2/92; where the importance of under-spin is considered, mainly from the point of view of stability of motion of a projectile which is always somewhat imperfect as regards both sphericity and homogeneity.

† "On the Motion of Projectiles," 2nd edn., London, 1890.

closely does the position of the vertex agree with observation; though it seems always considerably too near the middle of the path. But the calculated time of flight, which is greatest (for a given range) when there is no resistance, is always less than two-thirds of that observed:—while, for high speeds, and correspondingly high resistances, it is diminished to less than half the observed value. To make certain that this discrepancy was not due to the want of approximation in my equations, yet without the slightest hope of success in reconciling the various conflicting data, I made several calculations by the help of BASHFORTH'S very complete tables, which carry the approximation as far as could be wished; but the state of matters seemed worse rather than better. It then became clear to me that it is impossible for a projectile to pursue, for so long a period as *six seconds*, a path of only 180 yards, no part of which is so much as 100 feet above the ground:—unless there be some cause at work upon it which can, at least partially, counteract the effect of gravity. The only possible cause, in the circumstances, is *underspin*:—and it must, therefore, necessarily characterise, to a greater or less degree, every fine drive. (And I saw at once that I had not been mistaken in the opinion, which I had long ago formed from observation and had frequently expressed, that the very longest drives almost invariably go off at a comparatively slight elevation, and are *concave upwards* for nearly half the range.) In *Nature* (24/9/91) I said:—

“it thus appears that the rotation of the ball must play at least as essential a part in the grandest feature of the game, as it has long been known to do in those most distressing peculiarities called heeling, toeing, slicing, &c.”

This conclusion, obvious as it seemed to myself, was vigorously contested by nearly all of the more prominent golfers to whom I mentioned it:—being generally regarded as a sort of accusation, implying that the best players were habitually guilty of something quite as disgraceful as heeling or toeing, even though its effects might be beneficial instead of disastrous. The physical cause of the underspin appears at once when we consider that a good player usually tries to make the motion of the club-head as nearly as possible horizontal when it strikes the ball from the tee, and that he stands a little behind the tee. Thus the club-head is moving at impact in a direction *not* perpendicular to the striking face; and, unless the ball be at once perfectly spherical and perfectly smooth, such treatment must give it underspin:—the more rapid the rougher are the ball and the face of the club. This is, simply, NEWTON'S “oblique racket.”

In fact, if the ball be treated as hard, and if the friction be sufficient to prevent slipping, there is necessarily a *maximum* elevation (about 34°) producible by a club moving horizontally at impact, however much “spooned” the face may be. This maximum is produced when the face of the club makes, with the sole, an angle of about 28°:—which is less than that of the most exaggerated “baffy” I have seen. This, taken along with the remark above (*viz.* that the longest drives usually go off at very small elevations) is another independent proof that there is considerable underspin.

Hence the practical conclusion, that the face of a spoon, if it is to do its *proper* work efficiently, ought to be as smooth as possible.

2. I next considered how to take account, in my equations, of the effects of the rotation; and it appeared to me most probable that this could be done, with quite sufficient approximation, by introducing a new force whose direction is perpendicular at once to the line of flight and to the axis of rotation of the ball:—concurrent in fact with the direction of rotatory motion of the foremost point of the surface. Various considerations tended to show that its magnitude must be at least nearly proportional to the speed of rotation and that of translation conjointly. Among these there is the simple one that its direction is reversed when either of these motions is reversed. This may be generalised; for if the vector axis, ϵ , be anyhow inclined to the vector of translation, α , the direction (why not then the magnitude also, to a constant multiplier *près*) of the deflecting force is given by $V\epsilon\alpha$. Another is that, as the resistance (*i.e.* the pressure) on the non-rotating ball is proportional to the square of the speed, the pressures on the two front semihemispheres of the rotating ball must be (on the average) proportional to $(v + e\omega)^2$ and $(v - e\omega)^2$ respectively:—where v is the speed of translation, ω that of rotation, and e a linear constant. The resultant of these, perpendicular to the line of flight, will obviously be perpendicular also to the axis of rotation, and its magnitude will be as $v\omega$. But I need not enumerate more arguments of this kind. In the absence of anything approaching to a complete theory of the phenomenon we must make some assumption, and the true test of the assumption is the comparison of its consequences with the results of observation or experiment. This I have attempted, with some success, as will be seen below.

3. Another associated question, of greater scientific difficulty but of less apparent importance to my work, was the expression for the rate of loss of energy of rotation by the ball. Is it, or is it not, seriously modified by the translation? But here I had what seemed strong experimental evidence to go on, afforded by the fact that I had often seen a sliced or heeled ball rotating rapidly when it reached the ground at the end of its devious course. This is, of course, what would be expected if the deflecting force were the only, or at least the principal, result of the rotation:—for, being always perpendicular to the direction of translation, it does no work. But, on the other hand, if the *friction* on a rotating ball depends upon its rate of translation, the ball while flying should lose its spin faster than if its centre were at rest. This is a kind of information which might have been obtained at once from MAGNUS' experiments, but unfortunately was not.

4. As I felt that there was a good deal of uncertainty about the whole of these speculations, I resolved to consult Sir G. G. STOKES. I therefore, without stating any arguments, asked him whether my assumptions appeared to him to be sufficiently well-founded to warrant the expenditure of some time and labour in developing their consequences:—and I was much encouraged by his reply. For he wrote:—

“if the linear velocity at the surface, due to the rotation, is small compared with the velocity of translation, I think your suggestion of the law of resistance a reasonable one, and likely to be approximately true. This would make the deflecting force vary as

$v\omega$. I think too that the resistance in the line of flight will vary nearly as v^2 , irrespective of the velocity of rotation of the ball.

As to the decrement of the energy of rotation, I think the second law which you suggested is likely to be approximately true. The linear velocity due to rotation, even at the surface where it is greatest, being supposed small, or at least tolerably small, compared with the velocity of translation, I think you are right in saying that the force acting laterally upon the ball will vary, at least approximately, as $v\omega$. If this acted through the centre, it would have no moment. But I think it will not act through the centre, though probably not far from it, so that it would have a moment varying as $v\omega$. Hence the decrement of angular velocity would vary as $v\omega$, and the decrement of energy of rotation as $\omega (-d\omega/dt)$, or as $\omega \cdot v\omega$, or as $v\omega^2$, according to your second formula.

However, I think the force at any point of the surface, of the nature of that which we have been considering, would act very approximately towards the centre, and therefore would have little moment, so that after all the moment of the force tending to check the rotation may depend rather on the spin directly than on its combination with the velocity of translation. But, if this be so, I doubt whether the diminution of rotation during the short time that the ball is flying is sufficient to make it worth while to take it into account."

5. For a first enquiry, and one of great consequence as enabling us to get at least general notions of the magnitude of the deflecting force, let us take the simple case of a ball, projected in a direction perpendicular to its axis of rotation, in still air, and not acted on by gravity. [This would be the case of a top or "pearie," with its axis vertical, travelling on a smooth horizontal plane.] Suppose, further, that the rate of rotation is constant. Then, in intrinsic coördinates, the equations of tangential and normal acceleration given by our assumptions are

$$\ddot{s} = -\dot{s}^2/a, \text{ and } \dot{s}^2/\rho = \dot{s}^2 \frac{d\phi}{ds} = k\omega\dot{s},$$

respectively. The second may be put in either of the forms

$$\dot{\phi} = k\omega, \text{ or } \frac{d\phi}{ds} = k\omega/\dot{s}.$$

The first shows that the direction of motion revolves uniformly; the second, that the curvature is inversely as the speed of translation. And, as the first equation gives

$$\dot{s} = V \epsilon^{-s/a},$$

the intrinsic equation of the path is evidently

$$\phi = \frac{k\omega a}{V} (\epsilon^{s/a} - 1),$$

if ϕ be measured from the initial direction of projection, and V be the initial speed. This is an endless spiral, which has an asymptote, but no multiple points, and whose curvature is

$$\frac{k\omega}{V} \epsilon^{s/a}.$$

It therefore varies continuously from nil, at negative infinite values of s , to infinity at positive infinite values. Any arc of the spiral has therefore precisely the character of the horizontal projection of the path of a sliced, toed, or heeled, golf-ball; for it is obvious at once that the curvature steadily increases with the diminishing speed of the ball, thus far justifying the assumptions made in forming the equations of motion. We have only to trace this spiral, once for all, to get the path for *any* circumstances of projection. For the asymptote is obviously parallel to

$$\phi = -\frac{k\omega\alpha}{V} = -\alpha \text{ suppose.}$$

Measure ϕ from this direction, and the equation becomes

$$\phi = \alpha e^{s/\alpha}.$$

α gives the length corresponding to unit in the figure; and α (which determines the point of it from which the ball starts) depends only upon α and the *ratio* of the spin to the initial speed. This, with ϕ/α and s/α interchanged, is the equation of the equiangular spiral, which would be the path if the resistance were directly as the speed.

6. This enables us to get an approximate idea of the possible value of $k\omega$ in the flight of a golf-ball. For if it be well sliced, its direction of motion when it reaches the ground is often at right angles to the initial direction, although the whole deviation from a straight path may not be more than 20 or 30 yards. Assume for a moment, what will be fully justified later, that in such a case we may have (say) $s = 480$ feet, $\alpha = 240$ feet, and $V = 350$ foot-seconds. We see that

$$\frac{\pi}{2} = k\omega \times \frac{24}{35} \times 6.4;$$

so that

$$k\omega = \frac{\pi}{8.8} = 0.357, \text{ nearly,}$$

gives a sort of average value, which may safely be used in future calculations. In the case just considered, the acceleration (at starting) due to the rotation, is 0.357×350 or nearly four-fold that of gravity: *i.e.*, the initial deflecting force is four times the weight of the ball.

7. In trying to find the positions of the asymptote, and of the pole, of the spiral of §5, I spent a good deal of time on integrals like

$$\int_0^\infty \frac{\sin\phi \, d\phi}{\alpha + \phi};$$

with the hope of adapting them to easy numerical calculation by transformation to others with finite limits, such as $0 - \pi/2$. Happily, I learned from Professor CHRYSTAL that they had been tabulated by Mr J. W. L. GLAISHER;—and from his splendid paper (*Phil. Trans.*, 1870) I obtained at once all that I sought. In fact his $\text{Si}\phi$ and $\text{Ci}\phi$ are simply the x, y coördinates of this spiral (each divided by α); the axes being respectively the perpendicular from the pole on the asymptote, and the asymptote itself. Thus I traced at once, as shown in Fig. 1, the first three-quarters of a turn:—and the transformations I had

already obtained enabled me to interpolate points when (after $\phi = 5$) those given in the tables were too distant from one another for sure drawing. Another help in completing the curve graphically is given by the fact that the tangent, at any point, makes with the asymptote the angle ϕ which belongs to the point. This spiral does not, perhaps, exhibit the courses of the two functions so clearly as do the separate curves given by GLAISHER; but it certainly shows their mutual relation, and their maximum and minimum values, in a very striking manner.

The numbers, affixed to various points of the figured spiral are (in circular measure) the corresponding values of ϕ , or (by the equations of §5) they may be taken as proportional to the *times* of reaching these points by the moving ball, starting with infinite speed from an infinite distance.

8. Even in the plane problem of §5, the introduction of the effects of a steady current of wind in the plane of motion complicates the equations in a formidable manner. Suppose ϕ be measured from the reversed direction of the wind, and let the speed of the wind be W . Then if U , with direction ψ , be the *relative* velocity of the ball with regard to the wind, (for it is upon *this* that the resistance, and the deflecting force, depend), we have

$$\begin{aligned} U \cos \psi &= W + \dot{s} \cos \phi, \\ U \sin \psi &= \dot{s} \sin \phi; \end{aligned}$$

and the equations of motion are

$$\begin{aligned} \ddot{s} &= -\frac{U^2}{a} \cos(\phi - \psi) + kU \sin(\phi - \psi), \\ \frac{\dot{s}^2}{\rho} &= \frac{U^2}{a} \sin(\phi - \psi) + kU \cos(\phi - \psi); \end{aligned}$$

where, once for all, we have written k for $k\omega$.

Putting v for \dot{s} , and eliminating t , these become

$$\begin{aligned} v \frac{dv}{ds} &= -\frac{U}{a} (W \cos \phi + v) + kW \sin \phi, \\ v^2 \frac{d\phi}{ds} &= \frac{U}{a} W \sin \phi + k(W \cos \phi + v); \end{aligned}$$

where, of course,

$$U^2 = W^2 + v^2 + 2Wv \cos \phi.$$

These equations reduce themselves at once to the simpler ones above treated, when we put $W = 0$, and therefore $U = v$. As they stand they appear intractable, in general, except by laborious processes of quadrature. But while ϕ is small, *i.e.*, while the ball is advancing nearly in the wind's eye, they may be written approximately as

$$\begin{aligned} v \frac{dv}{ds} &= -\frac{(W+v)^2}{a} + kW\phi, \\ v^2 \frac{d\phi}{ds} &= \frac{W+v}{a} W\phi + k(W+v). \end{aligned}$$

From the first of these we see not only that the space-rate of diminution of speed is increased in the ratio $(W+v)^2/v^2$, which was otherwise obvious; but also that the rotation tends, in a feeble manner, to counteract this effect. From the second we see that the

space-rate of change of direction is increased, not only by the factor $(W + v)/v$ in the term due to spin, but by a direct contribution from the resistance itself. The effect of a head-wind in producing upward curvature, even in a "skimmer," is well known; and we now see that it is, at first, almost entirely due to the underspin which, without being aware of it, long drivers necessarily give to the ball. As soon as $\sin\phi$ has, by the agency of the underspin, acquired a finite value, the direct resistance comes in to aid the underspin in further increasing it. We now see the true nature of the important service which (in the hands of a powerful player) the *nearly vertical* face of a driving putter renders against a strong wind. It enables him to give great translatory speed, with little elevation, and with just spin enough to neutralize, for the earlier part of the path, the effect of gravity.

9. Before I met with ROBINS' paper, I had tried his pendulum experiment in a form which gives the operator much greater command over the circumstances of rotation than does his twisting of two strings together. Some years ago, with a view to measuring the coefficient of resistance of air, even for high speeds, in the necessarily moderate range afforded by a large room, I had procured a number of spherical wooden shells, turned very thin. My object, at that time, was to make the mass as small as possible, while the diameter was considerable:—but, of course, the moment of inertia was also very small. So, when I fixed in one of them the end of a thin iron wire, the other end of which was fastened to the lower extremity of a vertical spindle which could be driven at any desired speed by means of multiplying gear, the wire suffered very little torsion except at the moments of reversal of the spin. The pendulum vibrations of this ball showed almost perfect elliptic orbits, rotating about the centre in the same sense as did the shell:—and with angular velocity approximately proportional to that of the shell. These two experimental results are in full accordance with the assumed law for the deflecting force due to rotation. For, the ordinary vector equation of elliptic motion about the centre is

$$\ddot{\sigma} = -m^2\sigma.$$

If the orbit rotate, with angular velocity Ω , about the vertical unit vector α , perpendicular to its plane, σ becomes

$$\rho = \alpha^{2\Omega t/\pi} \sigma.$$

Eliminate σ from these equations, and we have at once

$$\ddot{\rho} = -(m^2 - \Omega^2)\rho + 2\Omega\alpha\dot{\rho}.$$

The part of the acceleration which depends upon the motion of translation of the bob:—viz.

$$2\Omega\alpha\dot{\rho},$$

is proportional to the speed, and also to Ω , that is (by the results of observation) proportional to the rate of spin; and it is perpendicular alike to α and to the direction of translation. These statements involve the complete assumption above. The other part of the acceleration depends upon position alone, and must therefore be $-n^2\rho$, that of the non-rotating ball. Hence we see that

$$m^2 = n^2 + \Omega^2,$$

or the period in the rotating ellipse is always shortened :—whether the ball move round it in the sense of the spin or not. *This* test cannot be applied with any certainty in the experiment described above, for in general Ω is much less than n , so that m exceeds n by a very small fraction only of its value.

A very beautiful modification of this experiment consists in making the path of the pendulum bob circular, before it is set in rotation. Then rotation, in the same sense as the revolution, makes the orbit shrink and notably diminishes the period. Reverse the rotation; the orbit swells out, and the period becomes longer.

10. The equations of motion of a golf-ball, which is rotating about an axis perpendicular to its plane of flight, and moving in still air, are now easily seen to be

$$\ddot{s} = -\frac{\dot{s}^2}{\alpha} - g \sin \phi,$$

$$\dot{\phi} = k - \frac{g}{\dot{s}} \cos \phi.$$

The most interesting case of this motion is a “long drive,” as it is called, where ϕ is always small, so long at least as it is positive; its utmost average value for the first two-thirds of the range being somewhere about 0.25. This applies up to, and about as much beyond, the point of contrary flexure. A little after passing that point, ϕ begins to diminish at a considerably greater rate than that at which it had previously increased.

A first approximation gives, as above,

$$\dot{s} = V \epsilon^{-s/\alpha},$$

if we omit the term $g \sin \phi$ in the first equation. With this, the second equation gives at once, on integration

$$\phi = \alpha + \frac{k\alpha}{V} (\epsilon^{s/\alpha} - 1) - \frac{g\alpha}{2V^2} (\epsilon^{2s/\alpha} - 1).$$

We might substitute this for $\sin \phi$ in the first equation, and so obtain a second, and now very close, approximation to the value of \dot{s} . But the result is far too cumbrous for convenient use in calculation. We will, therefore, be content for the present with the rudely approximate value of \dot{s} written above.

Integrating again with respect to s , we have

$$\int_0^s \phi ds = \alpha s + \frac{k\alpha^2}{V} \left(\epsilon^{s/\alpha} - 1 - \frac{s}{\alpha} \right) - \frac{g\alpha^2}{4V^2} \left(\epsilon^{2s/\alpha} - 1 - \frac{2s}{\alpha} \right).$$

Now, for rectangular coördinates (x horizontal) and the same origin,

$$x = \int_0^s \cos \phi ds = \int_0^s \left(1 - \frac{\phi^2}{2} + \&c. \right) ds, \quad y = \int_0^s \sin \phi ds = \int_0^s \left(\phi - \frac{\phi^3}{6} + \&c. \right) ds;$$

so that, to the order of approximation we have adopted, the equation of the path is

$$y = \alpha x + \frac{k\alpha^2}{V} \left(\epsilon^{x/\alpha} - 1 - \frac{x}{\alpha} \right) - \frac{g\alpha^2}{4V^2} \left(\epsilon^{2x/\alpha} - 1 - \frac{2x}{\alpha} \right).$$

The only really serious defect in this approximation is the omission of $g \sin \phi$ in the first equation. This renders the value of \dot{s} too large for the greater part of the path, and thus

the value of y will be slightly too small up to the point of inflection, and somewhat too large up to (and some way beyond) the vertex of the path.

11. When this paper was first read to the Society, it contained a considerable number of details and sketches of the paths of golf-balls, based on three very different estimates of the constant of resistance :—respectively much less than, nearly equal to, and considerably greater than, that suggested by BASHFORTH'S results. These details have just been printed in *Nature* (June 29), and I therefore suppress them here, replacing them by calculations based on experiments made *between* the two dates at the head of the paper. One important remark, suggested by the appearance of these curves must, however, be made now. Whatever, from 180 to 360 feet, be assumed as the value of α , the paths required to give a range of 180 yards and a time of 6^s·5, have a striking family resemblance. So much do they agree in general form, that I do not think anything like an approximation to the true value of α could be obtained from eye-observations alone. We must, therefore, find α or V directly. Only the possession of a really trustworthy value of α , found by such means, would justify the labour of attempting a closer approximation than that given above. I have not as yet obtained the means of making any direct determinations of α , but I have tried to find its value indirectly; first, from experimental measures of V made some years ago by means of a ballistic pendulum; secondly, a few days ago, by (what comes nearly to the same thing) measuring directly the speed of the club-head at impact, and thus determining the speed from the known coefficient of restitution of the ball. All of these experiments have been imperfect, mainly in consequence of the novelty of the circumstances and the feeling of insecurity, or even of danger, which prevented the player from doing his best. The results, however, seem to agree in showing that V is somewhat over 300 foot-seconds (say, for trial, 350) for a really fine drive. Taking the carry as 180 yards, and the time as 6^s, the value of α given by the formulæ above is somewhere about 240 feet. With these assumed data, the initial (direct) resistance to the ball's motion is sixteen-fold its weight. BASHFORTH'S results for iron spheres, when we take account of the diameter and mass of a golf-ball, give about 280 feet as the value of α . The difference (if it really exist) may possibly arise from the roughness of the golf-ball, which we now see to be essential to long carry and to steady flight, inasmuch as the ball is enabled by it to take readily a great amount of spin, and to avail itself of that spin to the utmost. One of the arguments in §2 above would give the resistance as proportional to $v^2 + e^2\omega^2$, instead of to v^2 simply.

12. We have thus all the data, except values of α and of k , required for the working out of the details of the path by means of the approximate x, y equation just given. The best course seems to be to assume values of α from 0·24 (according to Mr HODGE) down to zero; and to find for each the corresponding value of k which will make $y = 0$ for $x = 540$. This process gives the following values with $\alpha = 240$, $V = 350$, as above :—

α	k	kV/g	$a \log. kV/g$
0·24	0·182	2·00	166·3
0·12	0·246	2·69	237·5
0·0	0·309	3·37	291·6

It will be seen that the values of k are of the order pointed to by the behaviour of a sliced ball, though they are considerably less than that given in the example of §6. This, of course, is a strong argument in favour of the present theory; for, even in the wildest of (unintentional) heeling, the face of the club is scarcely so much inclined to its direction of motion as it is in good, ordinary, driving with a grassed club. (Slicing is very much less susceptible of accurate quantitative estimation by means of eye-observations.) The third column gives the ratio of the initial deflecting force to the weight of the ball. As this is more than unit in each of the three cases, all these paths are at first concave upwards. The numbers in the fourth column indicate (in feet) the distance along the range from the origin to the point of inflexion.

The approximate equation of the first of these paths is

$$y = 57.6 \frac{x}{a} + 30.05 \left(\epsilon^{x/a} - 1 - \frac{x}{a} \right) - 3.76 \left(\epsilon^{2x/a} - 1 - \frac{2x}{a} \right).$$

The abscissa of the maximum ordinate is given by

$$0 = 57.6 + 30.05(\epsilon^{x/a} - 1) - 7.52(\epsilon^{2x/a} - 1)$$

which leads to

$$\epsilon^{x/a} = 4.93, \text{ whence } x = 384 \text{ nearly.}$$

The vertex is therefore at 0.71 of the range.

13. Under exactly the same circumstances, *had there been no rotation*, the equation of the path would have been

$$y = 57.6 \frac{x}{a} - 3.76 \left(\epsilon^{2x/a} - 1 - \frac{2x}{a} \right).$$

This gives for $y = 0$,

$$x = 1.71 a = 410 \text{ feet only.}$$

The position of the vertex is given by

$$0 = 57.6 - 7.52 (\epsilon^{2x/a} - 1);$$

so that

$$x = 258 \text{ feet, nearly.}$$

In this case the vertex is at 0.63 of the range, only, and the time of flight is 3^s.1.

We have here, in consequence of a very moderate spin only, (in fact about half of that given by a good slice), all other initial circumstances being the same, an exceedingly well-marked difference in character between the two paths, as well as notable differences in range, and time of flight. Thus, while a player who gives no spin has (say) a carry of 136 yards only; another, who gives the *same* initial speed and inclination of path but *also* a very moderate amount of spin, accomplishes 180 yards with ease; his ball, in fact, remaining twice as long in the air.

14. For the sake of further illustration, let us consider the course by which the ball, sent off at the same inclination, but without rotation, may be forced by mere initial speed to have a range of 540 feet. Here the condition for V is

$$0 = 129.6 - 8 \left(\frac{240}{V} \right)^2 84.5,$$

so that the requisite speed is 548 foot-seconds; an increase of 56 per cent., involving about 2·5 fold energy of translation, which I take to be entirely beyond the power of any player. And the time of flight is reduced to 3^s·7 only, a rapidity of execution never witnessed in so long a carry. The initial resistance in this case rises to nearly forty-fold the weight of the ball. The equation of the path is

$$y = 57\cdot6 \frac{x}{a} - 1\cdot54 \left(e^{\frac{2x}{a}} - 1 - \frac{2x}{a} \right)$$

and the vertex is at 355, or about two-thirds of the range, only.

15. Fig. 2 shows the three paths just described, which start initially in the same direction; the uppermost is that with speed 350 and moderate spin. The lowest has the same speed, but no spin. The intermediate course, also, has no spin, but the initial speed is 548 to enable it to have a range of 540 feet. Thus the two upper paths in this figure are characteristic of the two modes of achieving a long carry:—viz. skill, and brute force, respectively. In fig. 3 the first of these paths is repeated, and along with it are given the corresponding trajectories with the same initial speed 350, but with inclinations of 0·12 and 0·0 respectively, and with the values of k , given above, which are required to secure the same common range. [To increase this range from 180 to 250 yards, even in the lowest and thus least advantageous path where there is no initial elevation, all that is required is to raise the value of kV (the initial acceleration due to rotation) from 108 to 219; i.e. practically to double it. V might, perhaps, be increased by from 25 to 30 per cent. by a greatly increased effort in driving:—but k is much more easily increased. A carry of 250 yards, in still air, is therefore quite compatible with our data, even if there be no initial elevation. It can be achieved, for instance, if V is 400 foot-seconds, and k about 50 per cent. greater than that which we have seen is given by a good slice. Of course it will be easier of attainment if the true value of a is greater than 240 feet. When there is no rotation there must be initial elevation; and, even if we make it as great as 1 in 4, the requisite speed of projection for a carry of 250 yards would be 1120 feet per second, or about that of sound.] Each of the curves has its vertex marked, and also its point of inflexion, when it happens to possess one. Fig. 4 gives a rough, conjectural, sketch of the probable form of the path if, other things being the same, the spin could be very greatly increased. As I do not see an easy way to a moderately approximate solution of this problem, either by calculation or by a graphic process, I intend to attempt it experimentally. I am encouraged to persevere in this by the fact that in one of the few trials which I have yet made, with a very weak bow, I managed to make a golf-ball move *point blank* to a mark 30 yards off. When the string was adjusted round the middle of the ball, instead of catching it lower, the droop in that distance was usually about 8 feet. With a more powerful bow, and with one of the thin wooden shells I have mentioned above, the circumstances will be very favourable for a path with a kink in it.

PROF. TAIT ON PATH OF A ROTATING SPHERICAL PROJECTILE.

Fig. 1.

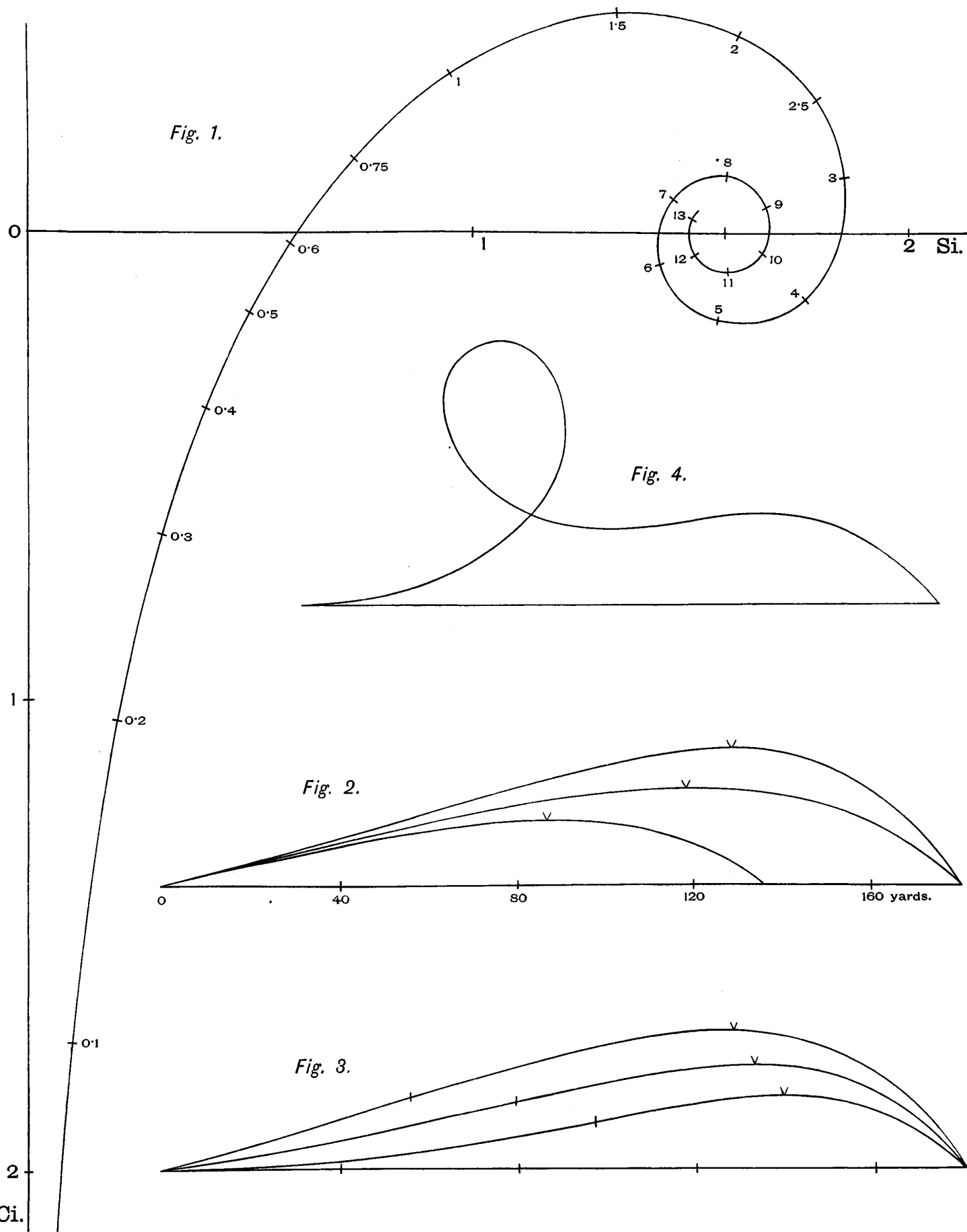


Fig. 4.

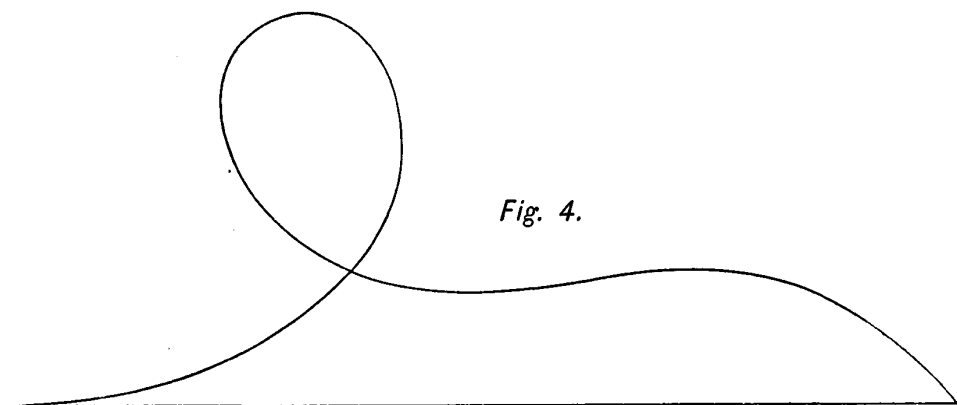


Fig. 2.

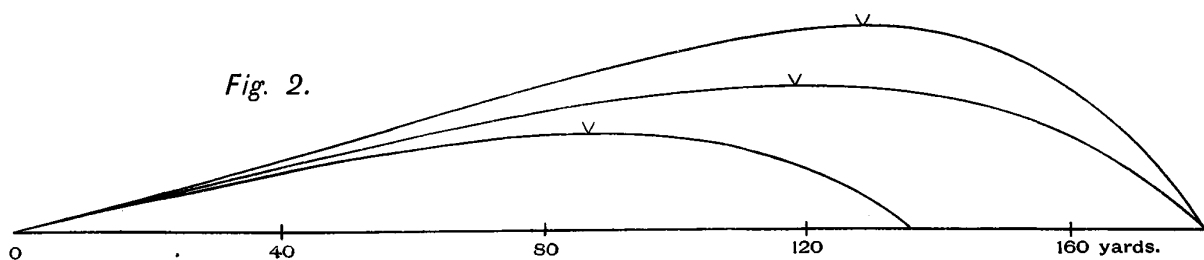


Fig. 3.

